EXPERIMENTS AND OBSERVATIONS
ON THE
SOUNDS OF THE HEART.

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As there is reason to believe, from the results of the last meeting of the medical section of the British Association, that investigations on that most difficult but interesting subject, the action of the heart, are now engaging the attention of physiologists, I beg, with all deference, to submit the following observations on the sounds of that organ to the consideration of these inquirers; not that they will find in them any thing like decided proof in support of any exclusive theory, but simply because they appear to be of such a nature, as, while they certainly do not throw unnecessary impediments in the way of the inquiry, may lead to more careful and less objectionable methods of investigation; perhaps to the elucidation of certain points on which there is so much difference of opinion; if not, to a certain extent, to the amalgamation of some of these views, so opposite in their nature, and yet so exclusively adopted by their various supporters.

In the month of July last, subsequent to some observations, made by the Edinburgh Committee of the British Association, on the action of the heart, in a horse, I took the opportunity of performing the following simple experiments, with the object of ascertaining whether or not a sound resembling either of the sounds of the heart, in health, could be produced by the stroke of that organ against the thorax, and in this manner testing the theory of Magendie, by assuming it as proved that the heart does strike the interior of the thorax during at least one of its movements; and, at the same time, taking into consideration the purely physical nature of such a phenomenon, and that a similar motion artificially produced in the dead body would most likely not materially modify the results.

Exp. 1.—For this purpose, immediately after the animal had expired, and the heart had ceased to contract, that organ was removed from the body. It soon thereafter became firm and contracted, and in this condition the following observations were made with it. The stethoscope was applied on the external surface of the thorax, and the heart, re-introduced within that cavity, was made to strike with its apex the inner surface of the thorax immediately underneath the situation of the instrument, while the rapidity and force of the strokes were made to imitate as nearly as possible the action of that organ, as seen and felt on experiments on living animals. At each stroke a distinct sound was perceived, in general louder according to the intensity of the former, and very much of the same character as the first or ventricular sound heard during life. Accompanying this, there was, of course, a sensation of impulse, which varied according to the intensity of the stroke against the chest. It was also observed, that a sound occurred at each removal of the apex of the heart from its pleural surface after its approach, and this was found to correspond exactly with the act of separation, and had very much the character, in abruptness and shortness, of the second or clear short sound heard during life. These observations were repeated many times with similar results.

Exp. 2.—In this experiment, which was performed in a dog poisoned for another purpose, the heart was retained in its pericardium, which was not the case in the first experiment. On the heart being made to strike the thorax, as already described, two sounds were distinctly perceived; the one, corresponding to the stroke against the chest, resembled the first sound of the heart, but was rather less grave than usual; the other, corresponding to the act of separation, sharp and abrupt. In this case, the heart was not so firmly contracted as in the former experiment. These observations were frequently repeated with similar results.

Exp. 3.—From the strictly physical nature of the phenomena described, analogy of structure might almost have sufficed for actual observation; but as direct experiment is at all times more satisfactory, and, in the present instance, so easy of execution, I took an early opportunity of repeating the foregoing experiments on the human body, and found similar causes to produce like results; two sounds being produced by the apex of the heart treated as described in the former experiments.

Exp. 4.—The abdomen of an adult was opened, as was also the stomach, while the thorax was left untouched. The fingers were now inserted into the œsophagus for the purpose, if possible, of making the heart strike against the anterior walls of the thorax; this, however, only produced an obscure rubbing and slight gurgling sound, from the mixture of mucus and air in the œsophagus, corresponding to the motion of the fingers, but not at all resembling the sounds produced in the other experiments; and it is much to be doubted whether the heart was made to strike the thorax at all in this experiment. On the pericardium being filled with water, however, by a small opening in the præcordial region, it was found easy, by an impulse from below, to make the heart strike against the chest, and produce the two sounds formerly described.

Exp. 5.—In another adult, the contents of the thorax were removed, and that cavity filled with water. In this state of parts the stethoscope was applied on the thorax externally, while percussion was made with the point of the middle finger,—which for this purpose answers better than the heart, unless firmly con-

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tracted,—on the internal surface of the chest underneath the water, and opposite the instrument. At each approximation of the finger to the parietes a sound was produced; as also on the act of separation, and simultaneous with it, a second sound was heard. In this instance the sounds were more subdued than in those in which the thorax contained air, and, in this respect, seemed to resemble still more those of the living heart; and, as was mentioned in the first experiment, the sound produced by the stroke was louder according to the strength of the impulse of the finger; but so little was required for the production of the sounds, that the mere quick approximation of the point of the finger to the thoracic parietes was sufficient for the development of the first sound; while the second appeared to owe its existence to the act of separation performed in the same rapid manner. Indeed, the intensity of the sounds seemed to depend more upon this than on the strength of the impulse.

Exp. 6.—Lest the peculiar structure of the point of the finger might have had any influence on the effects produced, that organ was in several instances covered with a portion of the membrane of the pleura, and in this condition most of the foregoing experiments were repeated with similar results. In the course of these observations on the human body, it was noticed that percussion on the intercostal spaces produced louder sounds than when this was performed on the ribs, when, however, the sounds seemed sharper.

In detailing these experiments, nothing is farther from my intention than to advance them as proving, that either of the sounds of the living heart, in health, are exclusively owing to the causes mentioned, as capable of giving rise to sounds of a somewhat similar character in the dead body; and, whether the latter have any share in the production of the former, may perhaps be deduced from the following observations, taken in connection with the foregoing experiments.

The first point in the inquiry demanding our attention, is contained in the following query: Does the heart during its action strike against the parietes of the thorax? Now, there is no difficulty in answering this question, as almost every writer on the subject admits, that during the contraction of the ventricles, the heart comes suddenly in contact with the parietes of the chest; and this is generally said to take place in the human body between the cartilages of the fifth and seventh left ribs, and is allowed to be the cause of the impulse perceived in that region. To show that this is not only admitted, but that the impulse is believed to be tolerably energetic, we may quote the observations of at once the most distinguished and most recent authors on the subject before us. Dr Hope informs us, that during the contraction of the ventricles, " the apex is tilted forwards and upwards, and occasions the impulse against the ribs;" and also, that " this motion is performed with considerable velocity."[*] Dr Williams says, " that the auricular contraction is immediately followed by the ventricular systole, which is accompanied by the first or dull sound. This systole, by straightening the anterior convexity of the ventricles, brings their apex into forcible contact with the ribs, and thus is produced the impulse."[†] Bouillaud admits, that the ventricular systole takes place suddenly, and is followed by the diastole. The contraction of the ventricles consists in a sudden energetic concussion of the point of the heart against the side.[‡] The Dublin Committee of the British Association conclude from their experiments, that " the ventricles, in their systole, approach the front of the thorax, and, by their contact and pressure against it, produce the impulse or beat of the heart."[§] Such are the inductions from the most recent investigations on this subject, and, from repeated observations on living animals, I have arrived at the same conclusions. It may be admitted, then, with little risk of error, that the heart during its action, not only approaches the thoracic parietes, but strikes against them with a certain degree of force, as implied by the terms shock, concussion, and impulse, in common use among physiological observers. It is also as generally admitted, and the proofs are equally strong, although we think it unnecessary to state them more at length, that the first sound of the heart accompanies the impulse, and is immediately followed by the second, which takes place during the ventricular diastole, and precedes the interval of silence.

Let us now inquire what evidence there is in support of the idea, that either of the sounds of the heart owe their existence, in any amount, to the percussion of the heart against the parietes of the thorax. According to Dr Williams, " sound in a physical sense may be defined as motion of a certain velocity resisted with a certain force;"[||] and we are informed by Bouillaud that, " the shock of the heart against the thoracic parietes is one of the conditions proper for the production of sound.[¶] Yet both of these authors entirely exclude this phenomenon as an element in the production of sound in the normal condition of the heart. But, possibly from seeing the difficulty of the position into which they have placed themselves, they afterwards admit the fact in certain anormal cases. For example, Dr Williams informs us, that " although it is proved by the preceding observations that the impulse of the heart against the chest does not produce its natural sounds, yet I am disposed to admit, that in violent action of the organ, its more sudden and abrupt strokes against the chest do cause a sound, which constitutes the loud termination of the first sound in these cases, and which seems nearer the ear, and more like a knock, than what is heard in the ordinary action of the heart. In common pulsations, the apex of the organ is drawn upwards and forwards at each systole, and sliding obliquely on the smooth pericardium, does not impel against the ribs with sufficient abruptness to cause sound. But in quicker and more violent pulses, the abruptness of the motion, and the force of the blow against the side of the chest, are such as can scarcely fail to produce sound."* According to Bouillaud, the percussion of the heart against the thorax does not produce any distinct sound in a normal state. But he conceives that " under the influence of very violent beatings it may be otherwise."†

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[*] Hope on Diseases of the Heart, 1835, p. 42–44.

[†] Rational Exposition, 1835, p. 165.

[‡] Bouillaud, Mal. du Cœur, 1835, i. p. 88.

[§] Report of Medical Section, 1835.

[||] Rational Exposition, 1835, p. 4.

[¶] Op. cit. p. 102.

To shew, however, that it is highly probable that sound is produced in the manner mentioned, in a much more extensive range of cases, if not, to a certain extent by this agency in every case, I beg here to refer to the foregoing experiments, by which it is distinctly proved that sound is produced by artificial percussion against the interior of the thorax, as well when that cavity contains air, as when filled with water; but, in the latter case, duller than in the former. Now, although neither of these conditions are proper to a healthy chest, still they represent opposite morbid conditions which not unfrequently occur, and in which the sounds of the heart are distinctly perceived, though sometimes modified; and it does not appear to me at all extravagant or unphilosophical to argue from these conditions, as from the chest in an entirely normal state. I think it likely that in the latter condition, if the experiment could be performed, the results would be somewhere between the two former; in which, in the case of the chest containing air only, the sounds were clearer than those produced under water, which again were somewhat duller than the sounds perceived in a normal condition of the parts in life. I am of this opinion from the fact of the lungs forming a physical state which may be considered the mean between the two conditions described. So much then in support of the principle laid down by Magendie, and to a limited extent supported even by Drs Williams and Bouillaud.

The next points of inquiry are, 1*st*, What degree or force of impulse is actually required for the production of sound under the circumstances mentioned; and is that of the healthy organ sufficient? 2*d*, What is the character of the sounds so produced; and do they at all resemble the normal sounds of the heart?

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* Op. Cit. p 178.

† Op. Cit. p. 193.

In answer to the first query, I again refer to the preceding experiments, in the account of which it is stated, that the degree of force required for the production of sound was very slight, and that a considerable sound resulted from the simple approximation of the point of the finger or apex of the heart, if of sufficient firmness, to the parietes of the thorax. That there is ample space in the human body for the heart to advance and retire from the walls of the chest, in the manner supposed by Magendie, appears not only from the foregoing observations, but likewise from the following consideration, viz. that the heart does not entirely occupy the pericardium, but is contained loosely in that envelope. But even this does not seem to be absolutely necessary to the motion of the organ, and in cases of universal adhesion of the pericardium to the heart, it appears to me quite possible for the organ,—projecting as it does into the left cavity of the pleura, and considering at the same time the great looseness of the cellular connections of the pericardium in the mediastinum,—to be tilted forwards and upwards, and to produce percussion of the thorax. And in such cases, we have not only the two sounds, but an impulse accompanying the first. I do not here allude to recent cases of pericarditis.

The amount of force required to produce sound in the dead body we have seen to be very small; and from the purely physical nature of the phenomenon, we have every reason to expect the same results from similar causes in the living body. Now, from frequent observation on dogs, I have not the slightest hesitation in stating, that the degree of impulse is considerably greater than was amply sufficient for the production of sound in the experiments previously recorded; and there is reason to believe, from comparative observations on these animals entire, and on the living human body, that the impulse of the heart is always stronger in the latter.

That the heart generally advances to, and retires to a certain extent from, the walls of the thorax during its action, may be considered as proved; but that it always takes place to the extent implied by the statements of Magendie, I think, may be questioned; at all events, from the following observations, this does not appear to be in all cases essential for the production of a certain degree of impulse. For example, in detailing one of his experiments, Dr Williams remarks, that "at each systole, the sudden tension or tightening of the ventricles was felt by the finger applied to their body, as an abrupt shock, with which the first sound exactly coincided."* And in an experiment on a dog performed last July, I find it stated in my notes, that while the finger was in close contact with the heart, the latter gave at each systole a sudden smart stroke to the former, and at each stroke a sound was perceived similar in character to the usual first sound. If an abrupt shock, then, can be produced against the finger under the circumstances mentioned, it is evident that the same may take place against the thorax, producing the impulse without necessitating the organ to retire much or even at all. It is probable that occasionally in enlargement of the heart, while a portion of the organ, of course the most anterior part, may constantly remain in contact with the parietes of the chest, the surrounding more posterior portions may actually advance and retire from the thorax during the action of the heart. It appears to me, then, from the foregoing observations, that the degree of force actually required for the production of sound under the circumstances mentioned, is by no means great, and that that of the human heart in health is amply sufficient.

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* Op. Cit. p. 171.

In answer to the 2d query, I beg also to refer to the same experiments. In these it is stated, that the sound produced by percussion against the interior of the thorax, very much resembled that heard during the systole of the ventricles, at which moment the heart strikes the chest; perhaps rather clearer in the case of the artificial pneumo-thorax. But this affords no valid objection, as it is not unusual to find the first sound altered, both as to length and clearness in a state of health; and on this point Bouilland remarks, that " he has observed in certain cases that the two sounds of the heart were nearly exactly alike, both as regards length and sound, and that it is in such cases the first which becomes clearer, and not the second which has lost its clearness. It is particularly in these individuals that the double sounds of the heart resemble the play of a machine with valves."*

Recent experimenters have endeavoured to prove, that the normal sounds of the heart are not at all owing to the impulse of the organ against the thorax, but in this they appear to me not to have succeeded. They doubtless have proved satisfactorily that sounds resembling those of the heart have been heard after a portion or the whole of the thoracic parietes had been removed, but not when all sources of sound, which might have resulted from percussion of the heart during its action, had been removed; and from all the observations I have made on this subject, it does not appear to be a matter of essential importance, that the heart should strike against the thoracic parietes for the production of sound. We at present confine our observations to the first sound, or that accompanying the impulse. In several of the experiments on dogs already alluded to, it was repeatedly observed, that when the heart was prevented from approaching the thorax by the interposed fingers, no sound was perceived by means of the stethoscope placed on the external surface of that cavity, provided the fingers were not permitted to touch the walls of the chest at the same time. When this was allowed, and these were placed close against the thorax, so as to form a portion of its parietes, as it were, a sound accompanying each impulse, was distinctly heard, of the same character as that perceived when the heart struck the thorax directly. As in this case then, the heart in most, if not all, of the experiments advanced against the views of Magendie, was permitted to strike against *something*, and this for the most part a firm body, in which instances we find that the intensity of the sound was scarcely altered; but when the stroke was against a soft body, the sound was always enfeebled. For example in the experiment of Bouillaud on a cock, we are told, that the sounds were heard by the stethoscope, applied directly over the heart; as also by the naked ear, a portion of cloth having been interposed to protect it from the blood, but in the latter case they were less distinctly heard. *(comme voilé.)** In Dr Williams' experiments, the stethoscope was applied close to the heart, except in observations 1 and 2, of experiment II, to be afterwards adverted to; and the Dublin Committee, aware of this difficulty in the inquiry, used in their experiments a stethoscope connected with a flexible tube and ear-piece, and in one experiment on a calf, they placed this instrument on the pericardium over the ventricles, and heard both sounds distinctly. In the experiments on the sounds with the sternum removed, they state, that "the flexible ear-tube was found to be serviceable in preventing the transmission of the shock or impulse which was felt when the common stethoscope was used, and which embarrassed the observation," (by the sound produced we presume). "The ear was now applied very near to, but not touching the heart, and both sounds were distinguishable but *feeble*. A small piece of board was placed over the surface of the ventricles, and kept in contact with the pericardium, and the common stethoscope having been applied to the surface of the board, both sounds were heard as distinctly, and very nearly as strongly, as when heard through the sternum."† In Dr Hope's experiments also the stethoscope was directly applied to the ventricles.

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* Op. Cit. Vol. i. p. 104.

That the sound produced by the percussion of the heart against a firm body, other than the thorax, is similar to the first sound, is evident from the following direct experiment of the Dublin Committee. "A calf having been stunned, the heart was taken out immediately, and placed on the table. The ear-tube was applied to the surface of the ventricles whilst they were still beating, and at each systole a sound was heard resembling that called the first sound; no second sound was audible."* In one of the experiments on a dog formerly alluded to, on the stethoscope being applied over the ventricles, two sounds were distinctly heard. Both were of the same character, and resembled the normal first sound. The one corresponded to the contraction, the other to the dilatation of the ventricles. In this case the heart was not removed from the body.

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* Op. cit. vol. i. p. 129.

† Report of Brit. Assoc. Med. Sect. 1835.

The experiment of M. Bouillaud on the cock, apparently discountenances the idea that percussion of the living heart against the stethoscope is capable of producing a sound like that of the first sound, perceived in ordinary circumstances. He says, that the *friction* of the heart against the stethoscope produced a single *bruit de frottement*, impossible to be confounded with the usual tic-tac of the heart.† But as no one, so far as I am aware, has advanced the notion that either of the sounds are owing to friction of the organ against anything, we cannot regard this experiment as militating at all against the views under consideration; and had the heart been permitted to tap or strike, instead of rub against the instrument, it is likely that a similar sound to that perceived in the former experiments, would have been heard in this case also.

With these facts before us, then, surely it is at least premature, and throwing obstructions in the way of free inquiry, and more accurate investigation, to draw the sweeping and decided conclusion, that the percussion of the heart against the thorax, in health, is incapable of producing any sound. In almost every instance noticed, the heart had something firm to strike against, which seems to be the most essential point; and it was observed in one case, that the sound was produced by the stroke of the organ even after it had been removed from the body; and this is an important fact, leading us at once to question the accuracy of the conclusions, derived from recent experiments, in which the heart *in situ* was permitted to strike against any, especially a firm object during its action.

In regard to those observations in which a portion of lung or cloth was interposed between the stethoscope or ear, as in the experiments of Williams and Bouillaud, it is evident that they only form an apparent exception to the foregoing statement, for, after compressing somewhat the soft interposed substance, the heart would meet, more or less according to the quality or quantity of the substance, with firm resistance; in the stethoscope in the one case, and the ear in the other; but, as in the familiar instance of muffling a knocker, we would expect the sound to be diminished by this arrangement, and such was the case, as noticed in the experiment of Bouillaud. Dr Williams does not inform us on this point, but only states, that "both sounds were distinctly heard through a lobe of the lung, interposed between the heart and the stethoscope."* I have tried the experiment in an artificial manner, by producing percussion on a portion of lung interposed between the stethoscope and the finger, and found a smothered resemblance, as it were, to the sound produced by percussion on the interior of the thorax. The experiments made with the flexible stethoscope of the Dublin Committee must also be regarded in the same light, for although the instrument might not transmit much of the impulse, it appears to me, that it would as easily transmit any sound resulting from the shock of the heart against its extremity, as any other instrument.

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* Op. cit.

† Op. cit. vol. i. p. 129.

But we may be told that both sounds have been heard when the heart struck against *nothing* in its motions:—let us consider this point. In the experiment of the Dublin Committee, already noticed, it is stated that the ear was applied very near to, but not touching the heart, and both sounds were distinguishable, but *feeble ;* and Dr Williams observed, in his 2d experiment, that "before the pericardium was opened, the 1st and 2d sounds were very distinctly heard, although the heart touched no part of the parietes of the chest."† And in summing up the evidence against Magendie's views from the same experimental testimony, he takes the rather unphilosophical liberty of stating, "that the same sounds that had been heard before the chest was opened, were most distinctly produced after that operation, when the heart struck against *nothing* in its motions."‡ Now, while we freely admit the possibility or even the probability of the existence of some other assistant cause for the production of the first sound, besides the percussion of the heart against the thorax, the experiments quoted do not prove that the sounds heard, which there is every reason to believe were feeble, did not in the present instances arise from the shock occasioned by the sudden contraction of the organ during the ventricular systole, on the immediately surrounding parts ; and after the experiment on the portion of lung, placed between the stethoscope and percussing finger, there is no improbability in the supposition. As to the condition of the heart, however, in this respect, we are not informed ; but as it is stated, that artificial respiration was performed in both, we of course infer the presence of the lungs ; so that when Dr Williams says that the heart struck against nothing in its motions, we presume he means that it did not strike against at least the anterior parietes of the thorax. It is mentioned that a portion only of the thorax was removed.

We may here notice an objection which has been offered by Dr Williams to the views advocated. In the Report of the medical section of the British Association, the learned author objected, " that if the sound depended on the impulse against the side of the thorax, the sound would be propagated all over the chest, according to a well known law in acoustics, and would be as audible over any part of the back as over the cartilages of the sixth or seventh rib."* Now, in every instance in which this can be done, investigations of a physical nature should be tested by experiment ; a mere theoretical statement is of no value in comparison ; and the instance before us is an example of the error likely to result from want of attention to this circumstance. It appears to me, that Dr Williams had never submitted this point to the test of experiment. If this had been the case, he would have arrived at a very different conclusion. I have repeatedly performed percussion, with the point of the finger, on the interior of the human chest as formerly described, as well when that cavity contained water, as air only ; and found that a degree of percussion, quite sufficient to produce a sound louder than is heard in many healthy adults, was perceived to a very small extent around the point of percussion ; nor were the resulting sounds heard distinctly many inches beyond this. For example, if percussion were performed in the space between the fifth and sixth ribs, and the stethoscope applied over the space between the fourth and fifth, or sixth and seventh ribs, the diminution of sound was very decided. Again, if the instrument was removed to the next space further off, the sound was in this instance perceived so feebly, that only an occasional beat, perhaps from being a little stronger than the rest, was heard. The sound in every instance, was found to diminish in intensity, in an inverse ratio to the distance from the site of percussion ; when this was performed on a rib, the sound was more easily communicated. Although, however, it is common for the sounds of the heart to be heard only in, or near the præcordial region, still it is an ordinary observation, that in a very great many instances they can be heard beyond this ; and in thin persons and in children, sometimes even over the whole of the thorax ; but always diminishing in intensity, as the examination is made progressively from the region of the heart.

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* Op. cit. p. 173. † Ibid. p. 173. ‡ Ibid. p. 178.

We have not taken notice of that theory which attributes the first sound of the heart to the sound of muscular contraction, described by Dr Wollaston, and in the present instance so strenuously supported by Dr Williams, without, as it appears to me, anything like satisfactory evidence in its favour. This idea is altogether rejected by Bouillaud and others, as an explanation of the first sound of the heart ; and it is somewhat curious to observe, as recently remarked, and shows the difficulty of the subject, that Drs Williams and Hope have arrived at different conclusions, as to the cause of the sound under consideration from the same experiments. The latter attributes it to three different causes, consisting, " 1*st*, possibly of a degree of valvular sound; 2*d*, of a loud smart sound produced by the abstract act of sudden jerking extension of the muscular walls, in the same way that such is produced by similar extension of the leather of a pair of bellows, and this he calls *the sound of extension;* 3*d*, a prolongation, and possibly an augmentation of this sound, by the sonorous vibrations peculiar to muscular fibre, *(bruit musculaire.")* He also remarks, that " the sound of the heart during palpitation is, in some instances, of such extraordinary intensity, that it would do violence to all analogy to suppose it produced solely by *bruit musculaire*, and some of the strongest advocates for this *bruit*, have thought it necessary in such cases to imagine a sound produced extrinsically by the heart impinging against the thoracic walls."*

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* Op. cit.

The Dublin Committee are of opinion, that the first sound " is produced either by the rapid passage of the blood over the irregular internal surfaces of the ventricles on its way towards the mouth of the arteries, or by the *bruit musculaire* of the ventricles, or probably by both these causes." Here the sound of muscular contraction is admitted as probably having a greater share in the production of the first sound than in Dr Hope's statement; but Dr Williams' conclusions are of a much more exclusive character; he attributes the first sound entirely to the *bruit musculaire*, and to those who object to this view, he puts the following question. " If the first sound of the heart is produced by another cause, what becomes of the muscular sound, in this case of rapid muscular contraction?"†

Now, to answer this question properly, it is necessary, in the first place, to know the degree of sound which muscles under contraction generally produce, before considering that produced during the contraction of the heart. As to the former we know, that it is generally a dull rushing sound, during continuous contraction, totally different from that of the first sound of the heart, in which organ, as the contractions can scarcely be said to be continuous, we do not look for this species of sound, and certainly do not find it. We have as yet, then, no proof except that arising from analogy of structure, which would lead us to suppose that it does take place in the heart, but, from being so short and feeble, is probably drowned in some other accompanying sound. Dr Williams indeed states, that " it is very possible to imitate the first sound of the heart by suddenly contracting the muscles of the gently closed hand; but it is not to be expected that the sound of muscles remaining in a state of tension can give a similar sound. These produce a dull rumbling sound described by Dr Wollaston."—" The muscular contraction of the heart is pre-eminently calculated to produce sound, inasmuch as the motion is strong, abrupt, and simple, and the resistance considerable, and continued as long as the semilunar valves are open, and the auricular shut."*

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* Hope.—Appendix, p. 7.

† Op. cit. p. 168.

The evidence then, on which Dr Williams supports his theory appears to consist chiefly in this, that the sound resembling the first sound of the heart can be produced by sudden muscular contraction, under circumstances in which the sound resulting might be much more simply, and, as appears to me, satisfactorily explained by the quick abrupt stroke of the contracting muscle either against the ear directly, or indirectly, against the stethoscope, and of all methods of producing sound, the simplest and most effectual is by a sudden stroke; and it seems to be trifling with the question to pass over in silence this manifest cause of sound, which has been so distinctly proved to exist in the previous observations; and that which is termed by Dr Hope the sudden jerking extension of the muscular walls of the heart,—and by Dr Williams the sudden tension or tightening of the ventricles, will manifestly assist in producing the impulse and consequent sound.

We are told by Dr Williams that " it is almost impossible to touch, stretch, bend, or compress solids, without throwing them into sonorous vibrations,"† and, as we have adopted that which is admitted on all hands as the simplest, and most evident cause of sound, so we cannot but regard the observations detailed in support of Dr Williams' theory, as more favourable to the views now advocated; and in concluding this part of the subject, we may be permitted to say, that, if an answer to Dr Williams' query is not to be gathered from the foregoing remarks, we would reply by putting the following question. If the first sound of the heart is produced entirely by the *bruit musculaire*, what becomes of that which we have the strongest reasons for believing must result from the abrupt stroke of the heart against the thorax, during the contraction of the ventricles?

The difficulties which lie in the way of this investigation, however, are beyond doubt very great, and while claiming consideration for the views of Magendie in regard to the cause of the first sound of the heart, it would be premature to come to any decided conclusion on the subject. At the same time I cannot avoid coming to the conclusion that the foregoing observations render it highly probable that the percussion of the heart, against the thoracic parietes, during the contraction of the ventricles, assists materially in the production of the first sound, if it be not the sole agent.

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* British and Foreign Med. Journ. No. ii. p. 447.

† Op. cit. p. 167.

The theory of Magendie in regard to the second sound, does not appear to me to deserve so much consideration; for, although it is quite true, and I have repeatedly verified the fact by experiment, that the ventricles, immediately after their contraction, dilate suddenly and with considerable force, yet they do not appear to strike the parietes of the thorax during this action. But, it is easy to understand how, under certain circumstances, they might do so with force sufficient to produce a very distinct sound; and we find that M. Bouillaud admits, that in an abnormal state, the diastole of the ventricles might give an appreciable impulsion against the chest.* Judging, however, from the number of the explanations offered, it is possible that there may be more than one cause for this phenomenon; and from the various experiments of Rouanet, Williams, Hope, Bouillaud, the Dublin Committee, and others, it appears highly probable that the quick unfolding and tension of the semilunar valves is the chief. From the experiments in the beginning of this paper, however, I am inclined to think, that the act of separation of the heart from the thorax, after its approach, and which in these observations was found to produce a sharp short sound, somewhat resembling the ordinary second sound, may in certain circumstances be an assistant cause. This will no doubt depend very much on the manner of separation, as well as on the nature of the substance which comes to fill up the space surrendered by the retiring heart, whether this be air, liquid, pulmonary tissue, or simply a fold of the pericardium.

In several cases the second has appeared to me to resemble very much the sound of separation, as heard in the experiment with air only, in the thorax; having a short ticking or clicking character. For the production of this sound, it does not appear necessary that the complete separation of the whole anterior surface of the heart from the walls of the thorax should take place; and in cases of enlargement of the organ, this, we have reason to believe does not happen. We know from experiment, that the separation of a portion, is sufficient for the production of the sound in the dead body; and the more extensive the surface of separation, perhaps the louder will be the sound.

If this, together with the previous view of the subject be correct, then we can understand how, in certain cases, there might be three or even four sounds heard during one revolution of the heart; (and M. Bouillaud has described several of these, in which, however, the sounds were partly of a morbid kind.) viz.

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* Op. cit. vol. i. p. 94.

the 1st, synchronous with the impulse of the apex of the heart against the thorax; the 2d, with the separation of that portion of the organ from the chest; the 3d, with the dilatation of the ventricles in such cases as give an appreciable impulsion against the thorax; and the 4th, with the separation of the walls of the ventricles from the chest, immediately after their dilatation. But as two of these may occur at nearly the very same instant of time, especially during quick action of the organ, so there might only be three or two sounds, as usually heard, in consequence of the union of two into one. And in ordinary cases, with perfect integrity of the semilunar valves, one at least of the above would, according to the theory which has been adopted as most probably the chief cause of the second, be completely lost in the valvular sound. These remarks are merely thrown out as points for the consideration of those engaged in this inquiry, should they be deemed the least worthy of their regard.*

I have several times endeavoured to test the views supported in this communication, and, at the same time the valvular theory of the second sound here adopted, in the following manner, but as yet without success, viz. by rapidly passing a ligature around the aorta and pulmonary artery, through a small incision in the pericardium, insinuating the ligature behind these vessels, by the natural opening, and tying them close to the semilunar valves. In every instance, however, the action of the heart, though vigorous before, became immediately so enfeebled, after the operation, as to afford no useful result.

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* In two instances I have heard three distinct sounds during one revolution of the heart; one of them, pointed out to me by my friend Dr Henderson, since writing the above, in a boy in the Royal Infirmary labouring under dropsy after Scarlatina, and in whom all the sounds were much alike, and more resembling the character of the second than that of the first, with a short pause after the third, thus, *tic–tic–tac—tic–tic–tac.* On examination after death, a small quantity of serous fluid was found in the pericardium, and the ventricles were somewhat dilated, especially the right.

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